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When Do Campaigns Matter? Informed Votes, the Heteroscedastic Logit and the Responsiveness of Electoral Outcomes

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Abstract

Previous research suggests that voters in mass elections tend to be badly informed. If these voters do not know enough about the relationship between the policy consequences of electoral outcomes and their own interests, then electoral outcomes may not provide meaningful expressions of voter interests. Can campaign activity affect the relationship between voter interests and electoral outcomes? To answer this question, we use survey data from 35 comparable elections and a new empirical methodology (Dubin and Zeng's [1991] heteroscedastic logit). The new methodology allows us to estimate the joint effect of voter information and interests on voting behavior in a way that is both theoretically justifiable and better at explaining the available data than traditional methods. We find that campaign activity increases the likelihood that electoral outcomes are responsive to (perhaps, otherwise badly informed) voter interests, when campaigners are able to exert costly and observable effort, are able to make credible statements and have the opportunity to engage in a vigorous and competitive campaign.

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1 Introduction

Over thirty years ago, Downs predicted (1957, Chapter 13) and Campbell, Converse, Miller and Stokes 1960 found that voters in mass elections were likely to be badly informed about political matters. These studies provided a generation of scholars with a new perspective about the existence and extent of voter ignorance. As a result, it is now a widely held belief that voters in mass elections tend to be poorly informed about the policy consequences of electoral outcomes. While there are many reasons to be concerned about voter ignorance, perhaps the most important is that voters may not know enough about the relationship between the policy consequences of electoral outcomes and their own interests (however defined) to provide electoral contestants with an incentive to be responsive to voter interests.

Whether an electoral outcome is a meaningful expression of voter interests or the product of unmitigated confusion, depends on the ability of voters to express their interests in the act of voting. If voters are truly unable to understand the policy consequences of electoral outcomes, then an election result should be treated as little more than the product of unmitigated confusion. If, on the other hand, voters can adapt to their uncertainty in a way that allows them to express their feelings about the type of future they desire, then an electoral outcome can legitimately be treated as an expression of voter interests. Which interpretation of an electoral outcome is appropriate depends on whether or not information sources like campaigns help voters understand the relationship between their interests and the electoral alternatives they face. Unfortunately, the informative capacity of campaigns is only partially understood.

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One set of scholars argues that campaigns allow relatively uninformed and uninterested voters to cast votes similar to those they would have cast if they were better informed about the policy consequences of electoral outcomes (this set includes Brady and Sniderman 1985, Feldman and Conover 1986, Lupia 1992, Popkin 1991 and Sniderman, Brody and Tetlock 1991). Each of these scholars begins by identifying a set of campaign events that could provide voters with cues about the consequences of electoral outcomes. Upon finding that some of these cues affect voter beliefs and/or behavior, they go on to argue that when campaigns provide these cues voters can overcome the problems associated with a lack of knowledge and interest in political matters. In short, these scholars argue that campaigns increase the likelihood that electoral outcomes reflect the interests that voters would express if they were well informed.

While these arguments are compelling, we often observe campaigns that are ineffective and more generally observe that campaigns differ in their ability to affect voters. Consider your own experience. Certainly, you have found some campaign claims to be more credible than others and some campaigners to be more persuasive than others. From some campaigners you have obtained reliable information about the policy consequences of electoral outcomes, while from other campaigners you have learned nothing. Whether or not campaigns are sufficient to convert electoral outcomes from the product of unmitigated confusion to a meaningful expression of voter interests seems to depend critically on particular characteristics of campaign activity. In what follows, we use both theoretical and empirical methods to identify the conditions under which campaigns do (and do not) lead to electoral outcomes that represent voter interests.

1.1 Research Plan

The goal of our research is to examine how campaign activity affects the relationship between voter interests and electoral outcomes – – a relationship we call *responsiveness*. We define responsiveness as a measure of the correspondence between the policy consequences of an electoral outcome and a voter's (or a group of voters) policy preferences. In defining our responsiveness variable, we recognize that the complexity of electoral issues can make voters uncertain about the types of candidates or policies that are consistent with their own best interests. The existence of this type of uncertainty implies that a voter's revealed preferences may not be the same as the preferences she would state if she understood the relationship between the policy consequences of electoral outcomes and her own well being. We call the preference a voter would have if she knew all that was known about the relationship between specific policies and her own well being as her *informed preference*.[1] We define an *informed vote* as the vote that a voter would cast if she was casting a vote consistent with her informed preferences. (Notice that to cast an informed vote a voter need not possess complete information or know all that is known about the consequences of her actions.) Finally, we define responsiveness with

[1]We assume that voters have preferences and that their level of informedness affects how they express these preferences. We expect that as a voter obtains the ability to link her actions with particular consequences, she becomes better able to express her informed preferences.

respect to a voter's informed preference, and say that the responsiveness of an electoral outcome increases as does the degree of similarity between the policy alternative that would receive her informed vote and the alternative that wins the election.

For instance, if my informed preferences lead me to favor Policy X over Policy Y, and Policy Y wins a referendum, we say that the electoral outcome is less responsive to my preferences than would have been the case had Policy X won the election. In contrast, if the informed preferences of an electorate were to lead 57% to favor Policy Y over Policy X, then we say that the electoral outcome is more responsive to the preferences of that 57% than would have been the case if Policy X had won. For expositional clarity, and without a loss of generality, we will, henceforth, use the term "responsiveness" to mean "responsiveness to the preferences of a majority of voters."

To identify which campaign characteristics are likely to affect responsiveness we first examine a spatial model that was developed in Gerber and Lupia 1992. In the model, interested parties send campaign messages to voters in an attempt to affect voting behavior. Voters observe certain types of campaign activity and may be able to use their observations to form relatively accurate inferences about the policy consequences of the possible electoral outcomes. Our primary findings from the spatial model are:

1. Voters can learn about the policy consequences of electoral outcomes from their observation of a campaigner's costly campaign effort (where the knowledge they obtain from this observation is independent of the content of campaign claims).

2. A campaigner's credibility affects what voters can learn from claims about the policy consequences of an electoral outcome.

3. An increase in the level of campaign competition leads to an increase in responsiveness if and only if the added campaigner is able to affect voter beliefs about the policy consequences of the electoral outcome.

4. In a broad range of cases, and all else constant, campaigns that argue against a change in the status quo are likely to have a greater impact on voting behavior than do campaigns that argue for such a change.

We then use campaign spending data and election results from 36 direct legislation elections and nearly 40,000 survey responses to questions about these elections to test hypotheses about effects of campaign activity on responsiveness. Our definition of responsiveness mandates that our estimation of this relationship be preceded by an estimate of "informed voting behavior." This task turns out to be our most challenging, and from the perspective of the empirical political scientist, perhaps the most interesting.

For initial motivation on how to estimate informed voting behavior, we turn to the logic of the spatial model which, like much of the recent work in political psychology (see Ferejohn and Kuklinski 1990 or Sniderman, Brody and Tetlock 1991 for reviews

of this research), suggests that all voters do not make decisions in the same way. We convert a primary theoretical finding into the following fundamental empirical premise: individuals who are relatively knowledgeable about the policy consequences of electoral outcomes are more likely to be casting their informed votes than those who are less knowledgeable. This premise leads us to assume that each voter's revealed preference over electoral alternatives (vote intention) is a noisy function of his or her informed preference. Therefore, we represent each voter's revealed preference as a single draw from a distribution whose mean is the voter's informed preference on that issue.

For any individual, we assume that the mean of her distribution is determined by measurable socioeconomic characteristics and election specific variables while the shape of her distribution is determined by how much she knows about the measure. From our initial premise it follows that relatively informed voters have distributions with smaller variances around the mean and that relatively uninformed voters have distributions with larger variances (or that a relatively well informed voter's revealed preference is closer to her informed preference than would be the case for a relatively uninformed voter). This formulation of voter choice presents us with a classic heteroscedasticity problem in which the error variance associated with using revealed preference is expected to decrease as a respondent's level of information increases and vice versa.

Since available data measures revealed voter preference as a binary variable (e.g. responses to vote intention questions), we employ a heteroscedastic logit model (developed in Dubin & Zeng 1991). This method of estimation differs from more traditional methods in that it allows us to estimate the relationship between relevant socioeconomic characteristics, voting behavior and (the degree of heteroscedasticity due to differences in) voter knowledge. Predicted values from the heteroscedastic logit allow us to estimate how respondents would vote if they were as well informed as the most informed respondents in the survey. These predictions provide our estimate of each voter's "informed vote." We then aggregate individual level informed vote estimates to obtain an estimate of the electorate's informed vote. Finally, we take the difference between our estimate of the informed electorate's behavior and the actual election result as our measure of responsiveness (e.g. the closer are informed and actual behavior, the greater is responsiveness and vice versa).

Our empirical results provide evidence that only certain types of campaign activity increase responsiveness and are consistent with the model's testable predictions. We find that an increase in the amount of campaign expenditure (our measure of costly campaign effort) increases responsiveness. This effect is greatest when the level of expenditure by opposing campaigns is relatively even. The implication of this finding is that large and hotly contested campaigns are more likely to increase responsiveness than are large one-sided campaigns. In addition, we find that, all else constant, the effect of negative campaigns (campaigns against change) are more effective than campaigns that advocate change. Finally, a comparison of our results to those produced by more traditional methods of statistical analysis shows that the heteroscedastic logit allows us to achieve a substantially superior fit of the data and allows us to draw improved theoretically justified inferences.

The result of our empirical and theoretical efforts is that we can identify which campaigns matter. The ability to take costly and observable effort, the ability to make credible statements and the opportunity to engage in a vigorous and competitive campaign are characteristics that allow campaigners to affect voter beliefs. When campaigners can affect voter beliefs in this way, they generally increase the likelihood that informed votes are cast. It follows that if campaigners possess the characteristics just mentioned, then campaigns are likely to represent voter interests. When campaigners do not have these characteristics, then electoral outcomes are more likely to represent unmitigated confusion.

Beyond any substantive merit that our findings possess, we believe that our research will be of interest to political scientists for two additional reasons. First, we are the first to use the heteroscedastic logit in a political science application and our experiences with it lead us to believe that this can be a useful method of estimation for, and a useful way to think about, empirical political science. Second, our research involves the integration of a formal theoretical model and rigorous empirical analysis. While the integration of theoretical and empirical principles is non-trivial, we believe that it is worthwhile and hope that the research experience we describe can provide a focal point for a constructive conversation about the benefits and drawbacks of attempting to confront a single question with these two seldom united paradigms.

Next, we briefly discuss the types of elections upon which our analyses focus. Then, we proceed to the analyses. We first describe the structure of and findings from the spatial model. Then, we conduct the empirical analysis. We conclude with a brief discussion of the policy implications of our findings.

1.2 Direct Legislation Elections

To examine the relationship between campaign activity and responsiveness, we focus on *direct legislation* elections.[2] Focusing on direct legislation allows us to study aspects of campaign activity that are difficult to isolate when focusing exclusively on candidate-centered campaigns. It is important to realize, however, that the many obvious similarities between direct legislation and candidate-centered elections make our findings generalizable to other sorts of campaigns in mass elections.

The features of direct legislation that make it particularly useful for a study of the effect of campaigns are:

- Direct legislation elections provide a unique and rich setting for analyzing the effect

[2]These can be either initiative measures, where citizens place a measure on the ballot by petition, or referendum measures, which are placed on the ballot by the legislature and ratified or rejected by the electorate.

of campaigns. Unlike recent presidential and other candidate centered elections, direct legislation campaigns tend not to be run by established political parties. Instead, they are typically run by groups that form for the sole purpose of taking a position on a direct legislation measure that appears on a particular ballot. As a result, there are types of measurable variance in the dynamics of direct legislation campaigns that simply do not exist in more commonly studied types of elections.

- Direct legislation voters select directly between an exogenously determined menu of specific policy alternatives, as opposed to selecting a candidate who acts as a policy agent. As a result and all else constant, the relationship between voter policy preferences and voting behavior is relatively simple in direct legislation elections.
- Direct legislation ballot measures tend to be long, technical, and complex (e.g. a recent California ballot measure contained over 15,000 words). As a consequence, voters often do not have a great deal of prior information about the electoral alternatives and must rely, to a greater extent than in candidate-centered elections, on political campaigns for information about the consequences of their election day actions. This reliance is reinforced by the regular absence of two types of cues that are commonly used by voters in candidate-centered elections: partisan endorsements and relevant past histories.
- Finally, data on voting behavior in direct legislation elections is readily available. Direct legislation elections occur regularly throughout the world, many in places that conduct survey research.

2 Theoretical Analysis

In this section, we present a brief, non-technical discussion of a spatial model of direct legislation developed in Gerber and Lupia 1992. This overview shows how we have conceptualized the relationship between campaign activity, voter information and responsiveness. In addition, this presentation is suggestive of the motivation for important components of our empirical analysis.

We model a direct legislation campaign as a one-shot game between a *proponent*, an *opponent* and n *voters*.[3] The object of the game is to select one of two policies, called the *status quo* and the *ballot measure*, that determines a payoff for all players. We assume that the proponent prefers the ballot measure to the status quo and that the opponent prefers the status quo to the ballot measure. Each of the two policies is represented as a point on the interval $[0, 1]$ and each player is assumed to have single-peaked preferences over policies (i.e. each has an ideal point and prefers policies that are relatively close to their own ideal point to policies that are relatively far).

The game has two distinct stages. In the first stage, the proponent and opponent choose campaign strategies. In the second stage, the voters observe campaign activity

[3]Our model can be characterized as a "setter model." See Rosenthal 1990 for a review.

and vote for either the ballot measure or the status quo. Majority rule determines which policy is selected. Unless otherwise stated, we assume that all aspects of the game are common knowledge.

We now state otherwise. An important feature of our model is our treatment of uncertainty. Every player is uncertain about the relationship between the actions they can take and the consequences of those actions. The source of voter uncertainty is the spatial location (which could also be referred to as the content or policy consequence) of the ballot measure. We assume that the spatial location of the status quo is common knowledge, while the location of the ballot measure is determined by a single draw from a common knowledge distribution.[4] We assume that the proponent and opponent observe this draw, but that voters do not.[5] This information asymmetry is justified by the following empirical observations:

1. Direct legislation campaigns are typically run by groups who understand a ballot measure well enough to organize an effort for or against it. Therefore, the proponent and opponent are characterized as being relatively well informed.

2. The content of ballot measures are often complex relative to what voters know about them at the beginning of a campaign. Therefore, voters are characterized as being relatively poorly informed.

In sum, the effect of our assumptions about voter uncertainty is that each voter can be uncertain about whether their informed vote would be for the status quo or the ballot measure.

The source of proponent and opponent uncertainty is the actual distribution of voter ideal points. We treat each voter's ideal point is as though it is drawn from a common knowledge distribution (although each voter's ideal point need not be drawn from the same distribution). We assume that every voter observes the result of only her draw. The effect of proponent and opponent uncertainty about any voter's ideal point (and, hence, the actual distribution of voter ideal points) is that neither knows exactly how their campaign strategy will affect the electoral outcome.

A second important feature of our model is our treatment of campaigning as a costly activity. The substantive motivation for this conceptualization is our observation of the money and effort that proponents and opponents expend when they attempt to affect the outcome of direct legislation elections (by waging a campaign). Our simplifying interpretation of these costs is that a cost is the amount that the proponent or opponent expects they must spend to run a "winning" campaign. Therefore, we assume that players do not choose the magnitude of their costs – a player's only decision is whether or not

[4]These assumptions represent voter uncertainty about the relationship between their own well being and the electoral outcome. Since the status quo is the existing state of policy, it is reasonable to assume that voters know more about it than the ballot measure.

[5]The case where some voters know more than this follows straightforwardly from our model.

to pay the costs she faces. For expositional simplicity, we also assume that once a cost is paid its magnitude is common knowledge.[6] In contrast, we treat voting as a costless activity.[7]

2.1 The Extensive Form Game

The sequence of events is depicted as an extensive form game in Figure 1. The proponent moves first and must decide to whether or not to wage a costly campaign.[8] In our model, a campaign consists of a message sent to voters about the content of the ballot measure. The proponent's campaign message is modeled as a simple left/right signal. The signal "left" is understood to mean that the proponent claims the ballot measure is to the left of the status quo on the interval $[0, 1]$. The signal "right" is defined accordingly. Since we recognize that campaigners may have an incentive to provide misleading information, we do not restrict the proponent to the transmission of a truthful campaign message.

Though the proponent is restricted to signaling either "left" or "right," the intuition provided by examining this type of communication is quite general. Since each player knows the location of her ideal point and the status quo, she has beliefs about whether she prefers points to the left or to the right of the status quo. Therefore, the messages "left" and "right" are equivalent to more general signals like "the ballot measure is better for you than the status quo" and "the ballot measure is worse for you than the status quo." We observe that many direct legislation campaign advertisements contain simple messages of this sort.

To wage a campaign, the proponent must first must commit to pay the "cost of campaigning in support of the ballot measure." The magnitude of this cost depends on the opponent's future actions, specifically whether she decides to wage an opposing campaign. We model the proponent's commitment to wage a campaign as a contingent contract where she agrees to pay one price if the opponent does not wage a campaign and another price if the opponent does decide to wage a campaign.[9] The effect of this assumption is to make the proponent uncertain about whether or not the opponent will also run a campaign.

After the proponent chooses a campaign strategy, the opponent observes the proponent's campaign activity and then decides whether or not to wage a campaign against

[6]Our results actually depend on the less restrictive assumption that all players have common prior beliefs about the magnitude of any cost.

[7]This simplification allows us to disregard possible differences between voters and non-voters. Since our definition of responsiveness depends only on the preferences of voters, it is without a loss of generality that we treat voting as a costless activity.

[8]In this model, we focus on the campaign process and treat the selection of the ballot measure location as exogenous. In Gerber and Lupia 1992 the determination of the ballot measure's location is treated as endogenous. In that model, it is necessary that the proponent move first, while in the current analysis, the order is irrelevant.

[9]We observe that in actual elections, the level of competition seems to affect a campaigner's choice of strategy, and, therefore, her costs.

the ballot measure. If the opponent wants to campaign against the ballot measure she must pay the exogenously determined "cost of waging an opposing campaign." Paying this cost allows the opponent to send the electorate a left/right signal, which is defined in the same way as the proponent's campaign message was defined. As with the proponent, we do not restrict the opponent to the transmission of a truthful campaign message.

Voters move last in this game. We assume that voters observe all campaign messages and know how much the proponent and opponent paid to wage their campaigns. Voters may, however, be uncertain about whether or not any particular campaign message is truthful.[10] Voters end the game by casting a vote for either the ballot measure or the status quo. Majority (or supermajority) rule determines whether the ballot measure or the status quo is the final outcome.

2.2 Findings

In the first part of this section, we describe the effect of observable costly campaign effort and campaigner credibility on voter beliefs about the policy consequences of electoral outcomes. In the second part, we describe the relationship between campaign activity and direct legislation responsiveness. All of the findings presented in this section are based on equilibria derived in Gerber and Lupia 1992.

2.2.1 Campaigns and Voter Inference

Let c^* be the cost actually paid by a campaigner (this discussion applies to either the proponent or the opponent). Because the campaigner need not pay c^* to attempt to affect the electoral outcome, the fact that she does so conveys information to voters. From the observation that the campaigner has waged a costly campaign, voters can infer that the ballot measure and the status quo must be far enough apart to justify her expenditure – – otherwise, she would accept the status quo and not expend c^*. Therefore, when a voter observes costly effort she learns that the ballot measure cannot lie within a specific range on the interval $[0, 1]$ that includes the status quo and points close to it. An example of this type of effect is displayed in Figure 2. A voter's ability to identify a range within which the ballot measure cannot lie allows her to form a more accurate inference about the spatial location (policy consequences) of the ballot measure. For example, a voter who is given a single opportunity to guess the exact location of the ballot measure – or which of a finite number of non-overlapping intervals the ballot measure lies within – before and after observing costly effort, is more likely to guess correctly after the observation. It follows that the larger the observable level of campaigner effort, the wider is the interval

[10]The assumption that voters can observe campaign effort and messages is without loss of generality to a class of assumptions where voter perceptiveness on these matters is impaired. We also make the simplifying and substantively justifiable assumption that voters are not sophisticated enough to draw a unique inference about the content of the ballot measure from the observation that the opponent does not wage a campaign in support of the status quo.

in which the voters know the ballot measure cannot lie and the more accurate are voter inferences, all else constant. Since the ability to cast informed votes requires accurate inferences about the policy consequences of an electoral outcome, a voter's ability to observe costly campaign effort increases the likelihood that an informed vote is cast.

Notice that the improvement in voter inferences due to the observation of costly campaign effort is independent of the content of campaign messages. Whether or not a voter can also use the content of a campaign message to learn about the policy consequences of an electoral outcome depends on her beliefs about its truthfulness, which itself depends on the campaigner's credibility. With respect to the issue of credibility, we recognize that in many collective choice situations, those who have the resources to provide information sometimes have an incentive to mislead those who receive it. To identify the effect of an information provider's credibility on voter inferences, we compare two extreme cases. In the first case, no voter is able to verify whether or not the content of a message is truthful (*a minimally credible campaign message*). In the second case, the content of a message is known to be truthful (*a perfectly credible campaign message*).[11]

In the case of the minimally credible campaign message, message content does not necessarily depend on the true directional relationship between the ballot measure and the status quo (campaigners can lie). Without additional information about the campaigner, it follows from Crawford and Sobel 1982 that voters cannot use the message's content to form a more accurate inference about the location of the ballot measure (it is regarded as "cheap talk"). Figure 3 provides a depiction of what voters learn from the content of a minimally credible campaign message – – nothing. When campaign costs are $c^* > 0$, however, the total effect of a campaign that includes a minimally credible campaign message is the same as that displayed in Figure 2. In this case, all of the change in voter beliefs is induced by the observation of costly campaign effort. In contrast, voters may be able to make improved inferences from the content of a perfectly credible campaign message. Voters can infer that the ballot measure is not to the right of the status quo when they receive the perfectly credible message "the ballot measure is to the left of the status quo." While this type of message is not sufficient for voters to infer the exact location of the ballot measure, it does allow them to eliminate ranges in which the ballot measure cannot lie. Figure 4 isolates the effect that the content of the perfectly credible campaign message "left" has on voter beliefs about the location of the ballot measure. Comparing Figure 4 with Figure 3 shows the effect of campaigner credibility on voter inferences. Notice that when an campaign message is perfectly credible, voters can generally form more accurate inferences about the location of the ballot measure than when the campaign message is minimally credible.

[11] A comprehensive study of the effects of credibility on the behavior of a direct legislation proponent and incompletely informed voters is the subject of Lupia 1993. A more general review of the effect of signaling on political decision making is provided in Banks 1991.

3 An Empirical Study of Responsiveness

We now estimate an empirical model that allows us to test for empirical evidence of the hypotheses relating campaign characteristics to responsiveness that our analysis of the spatial model generated. This section is divided into two parts. In the first part, we use data consisting of nearly 40,000 survey responses to estimate the responsiveness of 36 actual direct legislation elections. In the second part, we relate our estimates of responsiveness to aggregate data about campaign characteristics to estimate the determinants of responsiveness across elections.

The first and most difficult task is to estimate a measure of responsiveness. Our definition of responsiveness implies that a narrowing of the gap between actual aggregate direct legislation electoral outcomes and the electorate's informed vote is equivalent to an increase in responsiveness. It follows that creating a responsiveness measure requires knowledge of each voter's informed vote. However, most voters have limited information about political matters, making informed votes generally unobservable. As a result, the first step in the creation of a responsiveness measure is to estimate informed votes.

The intuition underlying our estimation of informed votes is derived from the spatial model. In short, this estimation is built on the (subsequently tested) premise that voters with higher levels of political information are better able to link the policy consequences of an electoral outcome to the actions they can take in the voting booth. Our estimation of an individual's informed vote, then, in a given direct legislation election, is derived from the relationship between an individual's level of information, her measured socioeconomic characteristics and her reported policy preferences (measured as her stated vote intention). Once we obtain estimates of each individual's informed vote, we use a straightforward aggregation procedure to estimate the electorate's aggregate informed vote. We then take the difference between our estimate of the informed electorate's behavior and the actual election result as our measure of responsiveness.

In the second part of our analysis, we test for evidence that campaigns with certain characteristics help to produce responsive direct legislation outcomes. We specify an empirical model that uses our measure of responsiveness as the dependent variable, and measures of campaign expenditure, competition and ballot measure characteristics as independent variables. The results of this analysis provide empirical support for the relationships between campaign activity and responsiveness identified in the analysis of the spatial model.

3.1 The First Task: Estimating a Measure of Responsiveness

We begin with the premise that individuals who are relatively well informed are more likely to be casting their informed votes. This premise leads us to assume that each voter's revealed preference over electoral alternatives, that is, her reported preference or vote intention, is a noisy function of her informed preference. The responses of individuals who

2.2.2 Determinants of Electoral Responsiveness

In equilibrium, a campaigner pays to wage an election if and only if she believes that doing so is necessary to ensure that the alternative she prefers wins the election. We also find that a campaign is competitive when at least one campaigner's prior beliefs about her opponent's ability and/or willingness to affect the electoral outcome, lead her to choose a different strategy than would have been the case if she had no such uncertainty. Finally, comparative statics reveal that the presence of campaigners who can exert observable and costly effort, or the presence of campaigners who voters believe to be somewhat credible, is generally sufficient to increase the likelihood that relatively uninformed voters cast informed votes. As a consequence, the presence of campaigners who can affect voter beliefs are necessary for electoral outcomes to be responsive to the interests of otherwise uninformed electorates.

The effects of campaign activity on responsiveness manifest themselves in three ways that can be tested empirically. First, we expect voters who observe higher levels of costly campaign effort to form more accurate inferences about the policy consequences of electoral outcomes than can voters who observe lower levels of campaign effort, all else constant. This implies that high levels of costly campaign effort should be associated with relatively responsive electoral outcomes.

Second, we expect any aspect of campaign activity that increases the credibility of campaign messages to also be associated with more responsive electoral outcomes. Since the credibility of campaign messages is often difficult to measure empirically, our test of this effect rests on the assumption that vigorous competition in the provision of campaign information increases the likelihood that a campaigner sends a truthful message. Supporting this assumption is our belief that the presence of a vigorous and able competitor, who is likely to have an incentive to expose untruthful campaign information, makes it more costly for a campaigner to send untruthful messages. If this belief is correct, then we expect that highly competitive and costly campaigns will increase campaigner credibility which will both increase the likelihood that informed votes are cast and be associated with greater responsiveness.

Finally, we expect campaigns against the ballot measure to be more effective than campaigns for the ballot measure. This expectation comes from our belief that voters are risk averse and that it is relatively easy for a campaigner to convince voters that the magnitude of the change (from the status quo) suggested by the ballot measure is large enough to deter risk averse voters from choosing the ballot measure. Our beliefs about the relative effectiveness of claims that "change is bad" are influenced by the fact that, in our model, even minimally credible campaigners can persuade voters of the magnitude of the difference between electoral outcomes as a result of her observable and costly effort. Our results are substantiated by the scholarship of Lowenstein (1982) and Jacobson (1983), both of whom suggest that voters treat the status quo (incumbents) differently than they treat agents of change (challengers).

have more information are assumed to be "contaminated" by less noise, and the responses of individuals with less information to be contaminated by more noise. Therefore, we represent each voter's revealed preference as a single draw from a distribution whose mean is the voter's informed preference on that issue. For any individual, we assume that the mean of this distribution is determined by measurable socioeconomic characteristics and election specific variables. The shape of this distribution is determined by how much the individual knows about the measure.[12] From our initial premise it follows that relatively informed voters have distributions with smaller variances around the mean and that relatively uninformed voters have distributions with larger variances. An implication of this premise is that we expect a relatively well informed respondent's revealed preference to be closer, on average, to her informed preference than would be the case for a relatively uninformed respondent.[13]

Figure 5 depicts the relationship between the theoretical and empirical concepts. In the spatial model, a voter's beliefs about the content of a ballot measure are represented

[12]Our assumptions about the relationship between information and informed preferences imply that the preference distributions of two individuals who have identical socio-economic characteristics and different levels of information will share a common mean but will have different variances. Supplemental analysis of the determinants of preferences for informed and uninformed respondents suggests that they are similar for the two groups, providing empirical justification for our assumption. Additionally, when we include a respondent's level of information as an explanatory variable in the supplemental analyses, we find little evidence of systematic differences in preferences between informed and uninformed respondents. These supplementary analyses are available upon request.

[13]Our approach is similar to the one used in Bartels 1990 to estimate "enlightened preferences." Bartels assumes that an individual's enlightened preferences are determined by her social position. Furthermore, for the purpose of estimating enlightened preferences, he assumes that a respondent's level of information and social position are independent. Given these two assumptions, which he recognizes as quite restrictive, Bartels estimates the relationship between policy preferences and social position using only the observations of respondents who the NES interviewer rates as "very high" in both "general level of information about politics and public affairs" and "apparent intelligence." He then uses coefficients from this estimation to calculate the enlightened preferences of both informed and uninformed respondents. For respondents who are not well informed, enlightened preferences are an estimate of the preferences they would have revealed if they processed information like a well informed person. Bartels' independence assumption is critical since there must be a sufficient number of well informed persons with low social position to ensure that the estimates of enlightened preferences are consistent.

While Bartels' approach is suitable for a study of preferences over general policy outcomes, we feel that it is less suitable for a study of preferences over specific electoral alternatives, like those considered in direct legislation. In particular, the complexity and specificity of the typical ballot measure, and the lack of traditional campaign cues, make us hesitant to adopt either Bartels' independence assumption or the assumption that uninformed voters would emulate the behavior of otherwise identical informed voters, if they themselves were well informed. Instead of assuming that well informed voters are expressing enlightened preferences, our model is based on the weaker assumption that well informed voters are *more likely* to express their informed preferences than are uninformed voters.

Our analysis allows information and social position to be related. In fact, our estimation of the determinants of an individual's level of information, described in Table 2, provides evidence that such a relationship exists. Our conceptualization of revealed preferences as draws from a distribution means that we need not assume that high information voters always "get it right" and reveal their informed preferences, and that low information voters systematically "get it wrong." Rather, we need only make the assumption (supported by evidence reported in Table 5) that high information voters are more likely to report their informed preferences on any single draw.

as a probability distribution over $[0, 1]$. We explained that an increase in a voter's ability to form an accurate inference about the content of a ballot measure is accompanied by an increase in the weight assigned to the true location of the ballot measure in the voter's updated beliefs. In general, more accurate voter inferences lead to an increase in the likelihood that the voter casts an informed vote. The intuition underlying our empirical concept is similar. As the variance around the mean of a distribution decreases, the weight of the probability assigned to the mean of the distribution (assumed to be the voter's informed preference) tends to increase, assuming that other parameters of the distribution remain constant. Therefore, as a voter becomes more informed, she is more likely to cast her informed vote. This formulation of voter choice presents us with a classic heteroscedasticity problem in which the error variance associated with using revealed preference as an estimate of informed preference varies systematically across observations. If our premise is correct, the error variance will decrease as a respondent's level of information increases and vice versa. In the empirical analysis that follows, we test for and detect this type of heteroscedasticity.

3.1.1 Estimating voter knowledge

Our first step in estimating informed preferences is to identify each individual's level of information. Our estimations of information level, informed vote, and responsiveness are conducted using data from a series of Field Poll surveys taken immediately preceding California (state-wide) primary and general elections from 1980 to 1990.[14] In these surveys, registered voters were asked a series of questions about some of the ballot measures in the upcoming election. Table 1 reports the number and title of each ballot measure for which questions were asked.

Respondents in each of the surveys were asked comparable questions about whether or not they had heard of each of the ballot measures listed in Table 1 for the election date following their interview. At best, binary responses to these knowledge assessment questions are imperfect measures of a respondent's level of information. However, we use these responses and the relatively weak assumption that respondents who report having heard of a ballot measure are likely to be better informed about it than respondents who report not having heard about it to obtain a measure of the respondent's level of information. To obtain this measure, we estimate a logit model that relates an individual's binary response to her measured socioeconomic characteristics, as follows:

$$\begin{aligned} info_{ij} &= p(know_{ij} = 1) \\ &= e^{X_{1i}\beta_1} / 1 + e^{X_{1i}\beta_1} \end{aligned} \tag{1}$$

[14]The Field Poll is an independent, non-partisan polling organization. Their surveys consist of telephone interviews with random samples of about 1000 registered California voters. Since the surveys sampled registered voters, not citizens, our inferences are limited to the former group. Field Poll data were obtained through the Social Science Data Base, University of California, San Diego.

Our unit of observation is an individual's response to the knowledge assessment question for a single ballot measure. The observed dependent variable, $know_{ij}$, is a binary variable that equals 1 if and only if respondent i reports that she has heard of measure j. The unobserved dependent variable, $info_{ij}$, is respondent i's level of information about proposition j, estimated as the probability that she responds "yes" to the corresponding knowledge assessment question.[15] X_{1i} is a vector of personal demographic characteristics.[16] Predicted probabilities of a yes response serve as our estimates of a respondent's level of information. Table 2 reports these logit coefficients.

3.1.2 Estimating Informed Votes

After Field Poll respondents were asked each knowledge assessment question, they were then read a brief summary of the corresponding ballot measure and asked to provide their vote intentions.[17] Like the knowledge assessment questions, vote intentions were coded as binary variables. We use our estimates of information level and responses to these vote intention questions to estimate each respondent's informed vote on the ballot measures about which he or she was asked.

[15]We realize that when vigorous campaigns are waged, respondents may be more likely to say they have heard of a measure, even if they have little or no content information. In other words, the likelihood that a respondent responds "yes" on the knowledge assessment question may be related to the level of campaign activity and independent of the information she actually has. Since we are using these responses to estimate level of information, which will ultimately be used to create our responsiveness measure, it is necessary that our estimates of information be independent of the level of activity in any particular campaign.

If the effects of a vigorous campaign systematically increase the likelihood of a "yes" response on knowledge assessment questions (that is, if level of information and campaign activity are *not* independent), then we will be less able to distinguish between informed and uninformed respondents (they will all appear to be informed). As a result, our estimates of informed preferences will be biased towards revealed preferences. This means that the relationship between our measures of responsiveness and campaign activity will increase and we will be unable to separate the effects of campaign activity on responsiveness from the effects of campaign activity on our ability to estimate informed preferences.

Therefore, we stack all of the knowledge assessment responses into a single data set and estimate a single model of information, in effect "averaging" over the effects of any specific campaign. This stacking procedure and our interpretation of the coefficient estimates are justified by results of separate logit estimations for each measure, as shown in Table 2. These separate estimations show great stability in the coefficients on each of the demographic variables and therefore indicate that the effects of these variables are very similar across ballot measures, independent of the measure's content. The constant terms, conversely, vary substantially across estimations.

[16]Included in this vector are the variables *Age*, which is measured as the respondent's age in years; *Educ* which is the number of years of education the respondent reports; *Gender* which is a dummy variable coded 1 for males and 0 for female; *Race* which is a dummy variable coded 1 for non-whites and 0 for whites; and *Owner*, which indicates home ownership and is coded 1 if the respondent owns his or her home and 0 otherwise.

[17]Our 36 cases are all ballot measures about which the Field Poll asked a comparable set of knowledge assessment and vote intention questions. Our cases do not include several ballot measures for which only respondents who answered affirmatively on the knowledge assessment questions were asked about their vote intentions. In the cases we examine, all respondents who were asked knowledge assessment questions were also asked their vote intention. Respondents who provided no vote intention were dropped from the sample.

Our estimation of informed votes follows directly from our initial premise: individuals who appear to be relatively well informed are more likely to be casting their informed votes. This premise suggests that the difference between a respondent's informed vote and their reported vote intention will be greater for respondents with lower levels of information and will be smaller for respondents with higher levels of information. It follows that when we analyze reported vote intentions for a population of respondents with different levels of information, then, all else constant, we expect to observe a smaller (greater) variance in the responses of the relatively informed (uninformed) respondents. In other words, we expect our error variance to be heteroscedastic.

To test for the existence and extent of heteroscedasticity and to use this information in generating our estimates of informed votes, we use the heteroscedastic logit model, developed in Dubin and Zeng 1992. This estimation procedure provides our estimate of informed votes by linking a respondent's reported vote intention with her measurable socioeconomic characteristics, recognizing the likely differences in error variance between respondents with different levels of information. If we find that relatively well informed respondents are more likely to be reporting their informed vote (i.e. if we find the type of heteroscedasticity the premise suggests), our procedure allows us to place more weight on their observations in our informed vote estimation procedure.

To estimate the heteroscedastic logit, we add a variance parameter to the standard logit specification to capture differences in the error variance across individuals. We specify the heteroscedastic logit as:

$$\begin{aligned} pref_{ij} &= p(vote_{ij} = 1) \\ &= e^{X_{2i}\beta_2\theta_i}/1 + e^{X_{2i}\beta_2\theta_i} \end{aligned} \tag{2}$$

where p is the probability that an informed voter with socioeconomic characteristics X_{2i} votes for a particular ballot measure. The heteroscedastic logit simultaneously estimates two sets of coefficients, one (β_2) that identifies how objective characteristics are related to reported vote intention across respondents, and a second (θ_i) that identifies how the relationship between informed vote and reported vote intention are related to a respondent's level of information. For identification, we parameterize the relationship between the variance in responses and information as an exponential function $\theta_i = e^{\alpha * info_i}$ and estimate α as the variance parameter in the logit estimation.[18]

In our application, the value of α reflects the level of error variance that, together with the coefficient estimates β, maximize the likelihood that the observed data are produced

[18]Since we are estimating both the structural parameters and the variance parameter from a single set of observations, identification is especially problematic. For example, a simple linear parameterization of θ such as $\theta_i = \alpha * \hat{info}_i$ is not likely to be identified. Dubin and Zeng prove that the identification conditions for the current parameterization of θ are met. Estimation of our model is facilitated using the Non-Linear Estimation procedure in Shazam. We specify the log- density as $v_i * log(1/(1 + e^{-X_i\beta\theta_i})) + (1 - v_i) * log(1 - (1/1 + e^{-X_i\beta\theta_i}))$ and estimate the parameters β and α. To verify the robustness of this procedure, we also estimate the model using the MLE routine in SST.

by the model. In other words, α is our estimate of the degree of heteroscedasticity. Positive, significant estimates for α suggest that the best model weights observations with higher levels of information more heavily in our estimation of informed votes. Positive α's are also evidence of the correctness of our initial premise.

Before we present the results of our estimation, it is instructive to consider two important differences between the relatively intuitive heteroscedastic linear regression that is often used in empirical political science and the heteroscedastic logit. One important difference is that the task of detecting heteroscedasticity is relatively difficult when using the logit specification. Consider the linear regression case where it is assumed that the dependent variable is accurately observed as a continuous variable. In that case, one can test for heteroscedasticity across individual observations by plotting the regression residuals against the level of information. In the non-linear case, however, it is assumed that the underlying continuous variable is only partially observed. The partial observation of the dependent variable forces us to make an explicit assumption about the distribution of the individual errors in order to make inferences about the true value of the underlying dependent variable from the observed discrete variable. (In logit estimations, we assume that these errors are drawn from a logistic distribution but can not observe these errors directly.) This explicit assumption implies that comparable residuals do not result from the logit estimation (they are subsumed in the logit model specification), and so the researcher must rely on more complicated detection tests, such as those described here.

A second important difference between the heteroscedastic linear regression and the heteroscedastic logit is that the consequences of uncorrected heteroscedasticity are much more severe in the non-linear logit case. In the linear case, the primary consequences of uncorrected heteroscedasticity are inflated standard errors, resulting in inefficient but still unbiased estimates. In the non-linear case, the resulting coefficient estimates may be inconsistent as well, meaning that in the limit, the distribution of the estimator does not collapse on the true parameter value.[19] In non-technical terms, this means that the estimated coefficients resulting from a logit estimation with uncorrected heteroscedasticity are different from the true parameters.[20]

[19]See Hanushek & Jackson 1977, pp. 340-341 for a discussion of consistency and other asymptotic properties of estimators.

[20]In the estimation of a logit model, the relationships between the explanatory variables and the continuous dependent variable (that is, the β's) are dependent on the assumptions we make about the relationship between the observed binary dependent variable and the nature of errors. In particular, we assume that the errors associated with the dependent variable are well described by a logistic distribution, and the probability that the binary variable takes on a particular value is determined by that distribution. In the heteroscedastic logit case, we relax the standard assumption that this error variance is constant across individuals. Instead, heteroscedasticity means that the relationship between the binary observation of the dependent variable (in the current application, the respondent's reported vote intention) and the underlying continuous dependent variable (informed votes) varies across observations. Since our ability to draw inferences about the relationships between the explanatory variables and the underlying dependent variable (from the estimated coefficients) is dependent on the assumptions we make about the errors, it follows that if the assumptions poorly describe the true underlying relationship between the observed and unobserved dependent variables, the inferences we can make (that is, the coefficients we estimate) are likely to be inaccurate. The relevance of the differences between maximum

Table 3 presents one set of coefficients from our heteroscedastic logit model (the β's). Patterns in these coefficients reflect differences in the relationship between the explanatory variables and preferences for different ballot measure topics. Table 4 presents the heteroscedasticity coefficients, α, that were estimated jointly with the effects reported in Table 3. From Table 4, we find strong evidence of heteroscedasticity. The values of α are generally positive and statistically significant which implies that relatively well informed respondents are more likely to be reporting their informed vote, as the initial premise predicts.

Notice that the last 12 cases, representing the propositions considered in November 1990, provide the weakest results. The reasons for this difference may be due to the unique design of that election's survey. Since there were so many propositions on the November 1990 ballot, the Field Poll split its sample and asked half the respondents about 6 of the propositions and the other half about the remaining 6. In addition, they further split each half-sample and asked the vote intention questions using 2 different formats, only one of which was consistent with the question format used in our other cases. Therefore, only about 250 respondents were asked their vote intentions in the comparable format. Given the complexity of our estimation procedures and the large number of parameters we are trying to estimate, it is not surprising that the results are weak and probably unreliable for these 12 cases. Subsequent analyses are run both with and without the November 1990 cases included.

We use the β coefficients from the heteroscedastic logit to produce our estimate of informed votes. To generate these estimates, we take the sum of [the individual's values on each of the explanatory variables in equation 2 $\times$ the estimated logit coefficient for these variables $\times$ the estimated weight parameter set to the value of the most informed respondent in the sample]. We can interpret these informed vote estimates as the vote intentions that a respondent would report if they were able to link their socioeconomic characteristics with their vote intentions as well as the most informed respondent in the sample (i.e. how the respondent would vote if they understood the policy consequences of the electoral outcome as well as the most informed respondent in the sample). Our informed vote estimates are subsequently aggregated across individuals for each measure and serve as our estimates of the outcomes that an informed electorate would produce. To facilitate comparison, Table 5 reports actual direct legislation outcomes, reported vote intention, and our estimates of informed votes. It is worth noting that respondents systematically over-report their intention to vote yes. In every case, the percentage of respondents that say they intend to vote in favor of a given proposition is higher than the percentage that actually do. This discrepancy might be attributable to the effect of differential voter turnout or the presence of risk-averse voters, who decide to stay with the status quo once they enter the voting booth. Unfortunately, we do not have sufficient data to identify the cause of this difference.

likelihood methods and linear methods for political science is explained in King 1989.

3.2 The Second Task: Estimating the Determinants of Responsiveness

We now estimate our model of responsiveness. The empirical model, detailed in the following equation, posits the responsiveness of direct legislation outcomes as a function of campaign activity and ballot measure characteristics.

$$\begin{aligned} Responsiveness &= \delta_0 + \delta_1 Spend_j + \delta_2 Spend_j * Comp_j + \delta_3 BigNo_j \\ &+ \delta_4 BigYes_j + \delta_5 Fiscal_j + U_j, \end{aligned} \tag{3}$$

where $Responsiveness = 1 - |Actual_j - Informed_j|$. $Actual_j$ is the actual percentage of voters who voted in favor of measure j. $Informed_j$ is our estimate of the percent of the electorate who would have voted in favor of measure j if they had cast informed votes. One minus the absolute value of the difference between $Actual_j$ and $Informed_j$ is our measure of responsiveness for measure j. From our definition of responsiveness, it follows that as the difference between the actual and informed outcomes decreases, *responsiveness* increases.

Our explanatory variables are intended to operationalize factors identified in the spatial model as affecting responsiveness. $Spend_j$ is measured as the total expenditure by the side of the electoral debate that spent the most money. We use this measure of campaign expenditure as a surrogate for the observable costly effort concept used in the spatial model.[21] In California direct legislation elections, money is spent by committees formed to support or oppose a particular measure and must be reported to the California Fair Political Practices Committee. *Spend* comes from the campaign expenditure figures published by this Committee and is divided by \$10,000,000 to scale. As shown in Table 6, campaign spending ranges from \$0 to \$24,284,000.

In our analysis of the spatial model, we found that if greater competition increased the credibility of the campaigners, then an increase in competition would lead to an increase in responsiveness. To test for any such effect, we add a second term, $Spend_j * Comp_j$, which conditions $Spend_j$ on the relative competition the spender faces. $Comp_j$ is defined as one minus the absolute value of the percent difference between spending for and against the ballot measure:

$$Comp = 1 - \left|\frac{\text{Total Proponent Expenditure} - \text{Total Opponent Expenditure}}{\text{Total Proponent Expenditure} + \text{Total Opponent Expenditure}}\right|$$

When the difference between spending for and against a measure is large (the campaign is effectively one-sided), our measure of *Comp* approaches 0. When the amount spent by

[21] While the magnitude of campaign expenditures does not always become a topic in pre-election conversation, we believe that real differences in campaign effort related to expenditure can be observed through noticeable differences in the number of paid advertisements.

each side is very close, the campaign is considered competitive and our measure of $Comp$ approaches 1.[22]

Except for the extreme case where both campaigners are minimally credible, our theoretical findings lead us to predict a positive and relatively large coefficient on $Spend*Comp$, since we believe that responsiveness is likely to be greatest when both sides are running vigorous campaigns. In addition, if, as predicted, observable and costly effort increases a voter's ability to form more accurate inferences about the policy consequences of electoral outcomes, then we also expect the total estimated effect of $Spend$, $[(Spend*Comp) + Spend)]$, to be positive.

Our theoretical analysis, Lowenstein's (1982) convincing argument that direct legislation voters who are exposed to extremely high amounts of negative (opponent) campaigning are more likely to vote for the already prevailing status quo and Jacobson's (1983) view that voters treat incumbents and challengers differently all suggest that large campaigns that oppose change are likely to have a greater impact than large campaigns that promote change. Two variables, $BigNo_j$ and $BigYes_j$ allow us to test this hypothesis. $BigNo_j$ is a dummy variable that indicates whether or not an opponent of the ballot measure waged a "significant" campaign. In 20 of the 36 cases we examine, opponents spent small amounts between \$0 and \$360,101. In the other 16 cases, opponent spending was much higher, ranging from \$1,135,240 to \$24,284,000. We score cases in the first group as having little or no substantial opponent spending and cases in the second group as having a serious opponent. $BigYes_j$ is similarly defined with significant campaigns being scored as those where spending for the ballot measure topped \$1.5 million.

The theory suggests that a big "No" campaign should have a greater effect on voting behavior than a big "Yes" campaign. We expect that as $BigNo$ increases, the percentage of the actual vote that the measure will receive should decrease. Since the informed vote is greater than the actual vote in each of the cases we examine, it follows that an increase in $BigNo$ should lead to a decrease in the value of the dependent variable (by decreasing $Actual$.) We expect the effect of $BigYes$ to be much smaller than the effect of $BigNo$ and to have the opposite sign (i.e. a big "Yes" campaign increases $Actual$.)

Our analysis of the spatial model also suggests that differences in the electorate's prior beliefs will affect their ability to determine the relationship between a ballot measure, the status quo, and their own well being. In the theoretical discussion, we assumed that voter prior beliefs were determined exogenous to the campaign. In the data, however, we recognize that differences in non-campaign stimuli may lead voters to form different types of prior beliefs across issues. Holding constant campaign attributes, we expect voters to be more interested in some types of issues than others. If interested voters are more likely to acquire relevant information, then they are more likely to be casting informed votes and electoral outcomes are more likely to be responsive, independent of

[22]We also included $Comp$ separately in an alternative specification of the model. We found that the independent effect of $Comp$ on *Responsiveness* as indistinguishable from zero, and so to preserve degrees of freedom, omit it from our final specification.

the effects of the campaign. Therefore, we include a measure of issue salience to account for the fact that voter interest and knowledge of particular ballot measures may vary.

Fiscal represents the California Legislative Analyst's prediction about the impact of the ballot measure on state revenues. *Fiscal* is scored 1 for measures that the Legislative Analyst identifies as having a substantial fiscal effect. This includes both revenue-increasing measures that entail either an increase in taxation or a reduction in some state service services, and revenue decreasing or spending measures. Our expectation (which, in this case, is not explicitly derived from the theory) is that voters pay more attention to fiscal measures – – a positive sign on ***Fiscal***.[23]

Table 7 reports the regression estimates of our empirical model of responsiveness. The first two rows of coefficients are the result of using the heteroscedastic logit to estimate informed voting behavior. To test the robustness of our results to the requirements of the heteroscedastic logit, we also estimated the responsiveness model using standard weighted logit procedures to estimate informed votes. A comparison of the first two and last four data columns not only shows that the relevant coefficients are relatively robust to model specification, it also shows that the heteroscedastic logit allowed us to draw inferences from a estimation that represented a superior fit of the existing data. In other words, the heteroscedastic logit is not only valued because of its relation to the theory, it also allows us to do a better job of drawing inferences from the data.

Column 2 reports the results of the responsiveness regression of primary interest and excludes the suspect November 1990 propositions. The effect of high and competitive spending ($Spend * Comp$) is positive, as expected. The effect of spending alone is also positive but smaller and not significant. These findings suggest that an increase in the magnitude of observable and costly campaign effort tends to lead to greater responsiveness and that this effect is greater when the campaign is competitive than it is when the campaign is one-sided.[24]

The presence of a strong opposing campaign also has the predicted effect. Notice that when opposition spending is high, the actual vote for the measure decreases, while high proponent spending has a much smaller effect. We take this as evidence for both our theoretical predictions about the effect of opposition spending and the Lowenstein/Jacobson risk-aversion hypotheses. Finally, and contrary to our expectations, the sign on *Fiscal* is negative and significant, suggesting that all else constant voters pay less attention to measures with important fiscal impacts. We suspect that qualitative differences between measures with and without substantial fiscal effects may be driving this result but leave further inquiry to future research.[25]

[23]Other variables we expect to be related to voter priors were included in alternative specifications of the model. However, none of these variables produced substantively or statistically significant effects. With only 36 observations, we excluded these variables to preserve degrees of freedom.

[24]The theory suggests that an increase in competition will lead to an increase in responsiveness when it increases either campaign costs or credibility. Unfortunately, we do not have sufficient data to determine whether either of these factors is driving the conditional effect of campaign expenditure.

[25]Column 3 reports estimates for the responsiveness regression including the November 1990 proposi-

4 Summary

We show that whether or not campaign activity can lead to an increase in responsiveness depends on whether or not there exist conditions under which campaigns increase the likelihood that a voter, who may not make a large investment in the acquisition of relevant information, can cast an informed vote. We identify these conditions as the existence of costly and observable campaign effort, credible campaigners and the level of competition, when competition increases the presence of one of the first two factors. It follows that when only when campaigns have these characteristics will campaigns lead to an increase in the likelihood that informed votes are cast and electoral outcomes are responsive to voter interests.

The policy consequences of our analysis are straightforward. More responsive electoral outcomes are unlikely to be produced by limits on campaigning. Simply restricting campaign activity by restricting expenditure will only ensure that confused voters remain confused. On the other hand, reforms that encourage campaign activity in order to encourage electoral contestants to be responsive to voter interests will only be successful if the campaigners have characteristics that allow voter to form more accurate inferences about the policy consequences of electoral outcomes.

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tions. (On three of these propositions, the heteroscedastic logit estimate lead to predictions that 100% of the respondents would cast affirmative informed votes. These propositions represented cases in which overall information was very high and thus the variance in our information variable was small. Given these extreme predictions and our existing suspicion of these results due to the small sample sizes for the November 1990 propositions, these three propositions are dropped from the analysis.) The main results from this regression are very similar to those reported in column 1. We find again that competitive spending is strong and significant. The independent effect of spending is again very small and insignificant, and in this case even slightly negative. The effect of a substantial "No" campaign is negative as in column 1, and the effect of a substantial "Yes" campaign is positive but not significant. Finally, fiscal measures are again associated with decreased responsiveness. The R^2 adjusted for degrees of freedom is lower than in the model in column 2, but still respectable for social science data.

Columns 4-7 report alternative specifications of the responsiveness variable. In these alternative specifications, informed votes are estimated using a simple weighted logit rather than the heteroscedastic logit. To estimate the weighted logit, we simply multiply all the variables for each observation (except the binary dependent variable) by the specified weight (as in a weighted regression) and estimated the logit model. Columns 4 and 5 report the results of the responsiveness regression in which informed votes are estimated using the weighted logit, with 2 different weighting schemes, excluding the November 1990 propositions. Columns 6 and 7 report the results of the responsiveness regression, using the same weighting schemes in a weighted logit estimation of informed votes, including the November 1990 propositions. In each case, the results based on the weighted logit are similar to those based on the heteroscedastic logit, except that the effects are typically smaller (the exception is the independent effect of spending), the standard errors on each estimate are higher, and the fit of the models is much worse. In fact, in for the models in columns 4 and 5, the R^2 adjusted for degrees of freedom is negative.

Election	Prop	Title
June 80	9	Taxation Income - Initiative
	10	Rent Control Initiative
	11	Taxation - Surtax - Initiative
Nov 80	8	Water Resources and Development
	10	Smoking - No Smoking Sections
June 82	1	New Prison Construction Bond Act
	7	Taxes - Income Tax Indexing
	8	Criminal Justice - "Victims Bill of Rights"
	9	Water Facilities - Peripheral Canal
Nov 82	9	Schools - Textbooks - Non-Public Schools
	11	Beverage Containers Initiative
	12	Bilateral Nuclear Freeze
	13	Water Resources Initiative
	14	Reapportionment
	15	Guns
June 84	24	Rules, Procedures, Powers, Funding
June 86	51	Multiple Defendants - Tort Liability Initiative
June 88	68	Legislative Campaigns - Spending & Contribution Limits
	69	AIDS Initiative
	71	Government Spending Limitations
	72	Emergency Reserve - Taxes to Transportation
	73	Campaign Funding
June 90	108	Passenger Rail & Clean Air Bond Act
	111	Traffic Congestion Relief & Spending Limitation Act
Nov. 90	126	Alcohol Beverage Tax
	128	Environment-Public Health Bonds
	129	Drug Enforcement, Prevention, Treatment, Prisons Bond
	130	Forest Acquisition Bond
	131	Limits on Term Offices, Ethics, Campaign Funding
	133	Drug Enforcement and Prevention
	134	Alcohol Surtax
	135	Pesticide Regulation
	136	State, Local Taxation
	138	Forestry Programs (Bond Act)
	139	Prison Inmate Labor (Tax Credit)
	140	Limits Term Office, Legislative Retirement, Legislative Operating Costs

Table 1: California Statewide Ballot Propositions

Prop	Const	Age/10	Educ/10	Gender	Race	Owner	N
Stacked	**-1.46**	**.11**	**.84**	**.25**	**-.15**	**.11**	38869
9	**-3.34**	**.34**	**2.76**	.13	-.12	.22	1155
10	**-1.99**	**.12**	**1.49**	.00	**-.33**	-.06	1152
11	**-2.26**	**.13**	**1.31**	.15	-.25	-.08	1160
8	**-3.45**	**.47**	**1.68**	**.59**	**-.65**	.13	975
10	.63	**.28**	.79	-.02	-.20	-.06	990
1	**-3.06**	**.26**	**1.04**	**.57**	.02	.25	1003
7	**-4.33**	**.22**	**1.62**	**.74**	-.24	**.38**	1003
8	**-3.04**	**.19**	**1.17**	**.37**	.08	.03	1003
9	-1.40	**.19**	**1.82**	**.56**	**-.61**	.17	1003
9	**-1.79**	**.17**	**1.12**	-.09	.01	.08	1922
11	.64	-.01	**1.59**	**.74**	**-.78**	**.12**	1922
12	-.29	.01	**1.83**	**.39**	**-.38**	.11	1922
13	-.30	.05	**.81**	**.38**	-.14	-.03	1922
14	-3.24	.16	1.73	.54	-.31	.35	1922
15	**2.90**	-.13	.87	**.70**	-.28	.00	1922
24	**-4.27**	**.30**	**1.85**	**.55**	**-.37**	.20	773
51	-.41	**.17**	**1.31**	.03	**-.57**	.21	1256
68	**-3.48**	**.23**	**1.04**	**.70**	-.17	.14	448
69	-1.13	.06	.04	.28	-.30	.10	443
71	**-5.16**	**.17**	**2.10**	.27	-.38	.52	439
72	-4.07	.33	.84	.46	-.28	.16	440
73	**-3.29**	.04	**1.39**	.24	-.51	-.25	438
108	**-1.44**	**.11**	**.86**	**.25**	**-.42**	**.33**	1153
111	**-2.68**	**.19**	**1.43**	**.30**	-.26	.24	1135
126	-.01	.12	.28	.17	.31	.13	533
128	-.42	-.03	**1.47**	.38	**-.85**	-.08	579
129	-.82	.09	.24	-.13	.11	-.39	521
130	.34	.03	.23	.06	**-.68**	-.31	550
131	**-2.57**	**.20**	**1.46**	**.57**	-.23	.21	535
133	**-2.62**	.08	.87	.17	.38	.03	545
134	.64	-.13	.66	.30	-.13	**.61**	576
135	-.68	.08	.54	.10	.21	.08	534
136	**-2.35**	**.19**	.49	**.50**	.03	-.09	530
138	-1.00	.07	**1.16**	.12	**-.64**	.01	526
139	**-3.17**	**.24**	**.90**	**.49**	.05	.20	562
140	**-3.88**	**.33**	**2.24**	.39	-.30	-.05	566

Table 2: Model of Information, Logit Estimates
Dependent Variable: Knowledge of Ballot Proposition
Bold faced entries: $p < .10$, two-tailed test

Prop	Const	Age/10	Gender	Race	PID	Union	Inc/10	Ideol	North	Urban
9	-.04	.00	.02	-.03	-.01	.00	.05	-.01	.00	.01
10	-.60	.01	.10	-.03	-.07	.01	1.09	-.10	.06	.08
11	.62	-.06	-.15	.13	.17	.22	-.60	.10	.10	-.08
8	.73	-.03	-.14	.11	.07	.02	-.80	.06	.01	.06
1	.05	.00	.01	.04	.02	.08	.51	.09	.01	-.04
7	.01	.00	.00	.00	.00	.00	.00	.00	.00	.00
8	7.56	-.06	-.81	2.44	-5.49	-2.23	5.20	-1.84	-1.38	.88
9	-.37	.05	-.17	.10	.01	-.03	.58	-.08	-.93	.21
9	.04	.02	-.03	.11	-.02	-.08	-.25	-.06	.02	.08
11	.00	.00	.00	-.13	-.01	-.05	.16	.22	.15	-.04
12	.40	-.03	-.16	.06	.13	.01	-.53	.34	.04	.11
13	.08	-.01	-.03	-.03	.00	.01	-.04	.05	.04	.03
14	-.12	.01	.08	-.07	-.04	-.07	.01	.01	.03	.05
15	-3.54	.38	-2.72	2.67	.49	-.65	-1.48	3.84	2.26	1.40
51	-.03	.01	.01	-.02	-.03	-.03	.13	-.02	.02	-.03
68	.37	.06	-.06	.03	-.21	.17	-.18	.26	.42	.18
69	-.12	.02	.02	.00	-.02	.01	-.07	-.05	-.01	-.03
71	.36	-.08	-.36	.22	.19	.13	.22	.46	.18	.17
72	.28	.03	.09	-.023	-.22	-.25	-.17	-.16	-.17	.04
73	.03	.00	-.01	-.01	.00	.00	.00	.01	.01	.01
108	1.75	-.23	.11	-.08	.29	***	.35	.55	.05	.42
111	.97	-.13	-.03	.30	.18	***	.23	.14	.07	.08
126	.69	.109	-.69	.44	.28	***	-.09	.21	-.11	-.55
128	.63	-.36	-.43	-.30	.83	***	-.93	.46	-.09	.38
129	3.62	-.02	-.10	-.58	.52	***	.37	-.07	.11	-.13
130	.37	-.20	-.15	-.19	.34	***	-.18	.49	.07	.38
131	-2.10	-.15	.03	.11	.22	***	-.26	-.58	.28	.62
133	2.33	-.02	-.35	-.36	.21	***	.88	-.28	-.16	.38
134	-.17	.07	-.12	.10	-.07	***	-.10	.26	-.28	.01
135	-1.14	-.09	.22	.44	.16	***	-.61	.51	.20	.25
136	.41	.02	.16	-.46	-.14	***	-.56	-.40	.04	.22
138	4.27	-.25	.12	.58	.20	***	-.18	.11	.01	.47
139	.95	-.01	-.36	-.62	.21	***	.66	-.88	.26	.93
140	2.05	-.09	-.28	-.55	-.28	***	-.35	-.18	.10	.17

Table 3: Model of Informed Preferences, Heteroscedastic Logit Estimates
Dependent Variable: Reported Vote Intention
$\theta = exp(\alpha * Info)$

Prop	Estimated α	Standard Error
9	**7.4275**	(1.4498)
10	2.2035	(1.6892)
11	1.5249	(1.8433)
8	**2.6972**	(1.2558)
1	1.9448	(1.8702)
7	**7.7128**	(4.3043)
8	**-3.5818**	(1.5265)
9	**1.7674**	(.9109)
9	2.0058	(1.8680)
11	1.5640	(2.3469)
12	**2.1078**	(.9462)
13	**4.5512**	(2.0406)
14	**3.8206**	(2.0249)
15	**-3.4900**	(1.3370)
51	**3.9916**	(1.6308)
68	.6455	(2.4149)
69	**4.7315**	(1.5957)
71	1.3576	(1.7523)
72	1.2565	(3.4965)
73	**6.2930**	(2.2241)
108	1.2061	(.7525)
111	1.0397	(.6429)
126	.5108	(3.1116)
128	-2.4347	(3.3564)
129	5.7721	(3.6228)
130	-1.0364	(3.4209)
131	-4.0016	(3.3276)
133	4.1899	(3.2293)
134	-.8116	(3.1449)
135	-1.7601	(3.5675)
136	1.2607	(3.3594)
138	**6.2979**	(3.5278)
139	1.2248	(3.8201)
140	.8989	(3.3948)

Table 4: Model of Informed Preferences, Heteroscedastic Logit Estimates of α
Estimated Jointly with Table 3 Estimates
$\theta = exp(\alpha * Info)$
Bold faced entries: $p < .10$, two-tailed test

Prop	Actual	Reported	Informed
9	.3920	.3884	.1813
10	.3540	.3871	.3202
11	.4430	.6053	.6455
8	.5380	.8100	.9006
10	.4660	.5451	****
1	.5610	.6318	..6969
7	.6350	.5774	..8597
8	.5640	.7092	.6159
9	.3730	.3933	.4048
9	.6110	.5738	.6107
11	.4410	.4962	.5075
12	.5230	.5345	.5829
13	.3520	.5757	.6750
14	.4550	.4694	.4269
15	.3720	.4186	.4542
24	.5310	.4816	****
51	.6210	.5966	.5961
68	.5280	.7814	.8113
69	.3200	.2762	.1487
71	.4890	.5772	.6049
72	.3850	.6460	.6706
73	.5810	.6683	.8891
108	.5600	.6568	.9047
111	.5200	.5529	.7887
126	.4092	.5305	.6238
128	.3565	.4643	.4444
129	.2766	.4867	.9999
130	.4787	.5234	.4443
131	.3775	.5516	.4990
133	.3187	.5318	.9999
134	.3103	.5800	.4913
135	.3040	.4025	.4114
136	.4788	.4506	.7569
138	.2884	.4528	.9999
139	.5405	.7245	.9694
140	.5217	.7093	.9375

Table 5: Actual Direct Legislation Returns, Reported Vote Intention, and Estimated Informed Preferences

Prop	SpendYes	SpendNo	BigNo	Fiscal
9	3,633,570	1,777,740	1	1
10	6,655,210	178,271	0	0
11	455,899	5,611,460	1	1
hline 8	62,402	360,101	0	0
10	797,133	2,732,010	1	0
1	0	30,729	0	1
7	475,664	1,074	0	1
8	90,842	54,286	0	1
9	2,789,920	3,184,880	1	1
9	595,489	10,175	0	0
11	666,975	5,133,860	1	0
12	2,627,270	6,041	0	0
13	624,607	2,028,760	1	1
14	222,654	0	0	0
15	1,781,270	6,367,470	1	0
24	96,610	329,723	0	1
51	4,878,830	4,930,050	1	1
68	1,038,760	1,135,240	1	1
69	170,377	279,844	0	1
71	2,519,520	244,564	0	1
72	2,832,820	199,942	0	1
73	335,136	1,135,240	1	1
108	452,262	0	0	1
111	6,490,700	0	0	1
126	3,299,410	0	0	1
128	5,737,870	13,116,800	1	1
129	1,100,180	20,387	0	1
130	7,079,960	5,608,950	1	1
131	1,164,610	2,347,750	1	0
133	802,581	0	0	0
134	1,896,900	24,284,000	1	1
135	5,681,160	0	0	1
136	11,023,000	95,886	0	0
138	5,608,950	7,079,960	1	1
139	1,326,110	282,122	0	1
140	1,953,700	2,347,750	1	1

Table 6: Model of Direct Legislation Responsiveness, Explanatory Variables

Variable	Col1	Col2	Col3	Col4	Col5	Col6
Constant	**.9011**	**.8839**	**.9200**	**.9205**	**.9311**	**.9314**
	(.0521)	(.0472)	(.0387)	(.0390)	(.0322)	(.0324)
Spending	.0490	**-.0084**	.0672	.0674	-.0627	-.0623
	(.1633)	(.0543)	(.0371)	(.0373)	(.1267)	(.1278)
Spend*Comp	**.3125**	**.2133**	.1668	.1662	.1026	.1026
	(.1589)	(.0987)	(.1230)	(.1240)	(.0675)	(.0679)
BigNo	-.0951	-.0595	-.0731	-.0723	-.0490	-.0484
	(.0772)	(.0590)	(.0574)	(.0579)	(.0403)	(.0405)
BigYes	-.0091	.0128	-.0063	-.0065	.0459	.0459
	(.0781)	(.0545)	(.0601)	(.0607)	(.0372)	(.0374)
Fiscal	**-.1157**	**-.1144**	-.0282	-.0292	-.0504	-.0513
	(.0526)	(.0474)	(.0383)	(.0386)	(.0324)	(.0326)
N	22	31	24	24	31	31
R^2	.2181	.1565	-.0342	-.0376	.1141	.1120

Table 7: Model of Direct Legislation Responsiveness, OLS Estimates
Dependent Variable: Responsiveness $= 1 - |Actual - Informed|$

Col 1: Excludes November 1990 Propositions, $\theta = exp(\alpha * Info)$
Col 2: Includes November 1990 Propositions, $\theta = exp(\alpha * Info)$
Col 3: Excludes Nov 1990, Informed votes estimated using weighted logit, $w = Info$
Col 4: Excludes Nov 1990, Informed votes estimated using weighted logit, $w = exp(Info)$
Col 5: Includes Nov 1990, Informed votes estimated using weighted logit, $w = Info$
Col 6: Includes Nov 1990, Informed votes estimated using weighted logit, $w = exp(Info)$

Bold faced entries: $p < .05$, two-tailed test

FIGURE 1:
Extensive Form Game

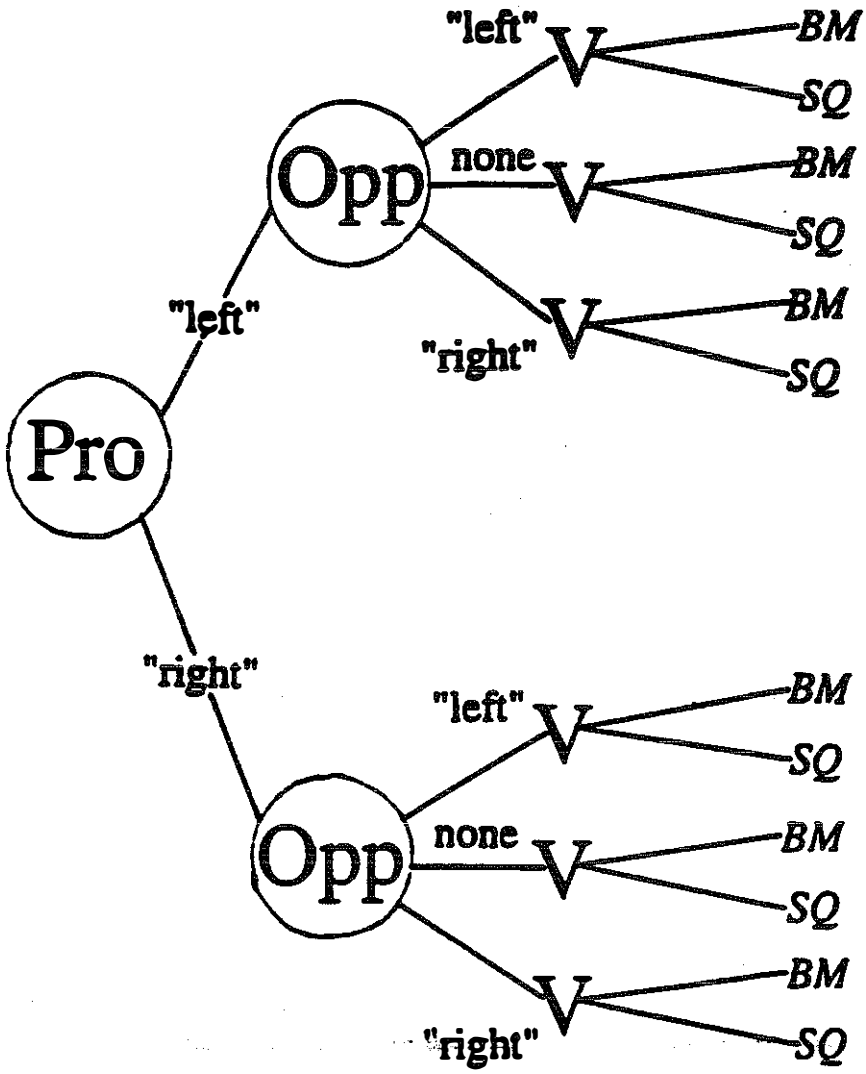
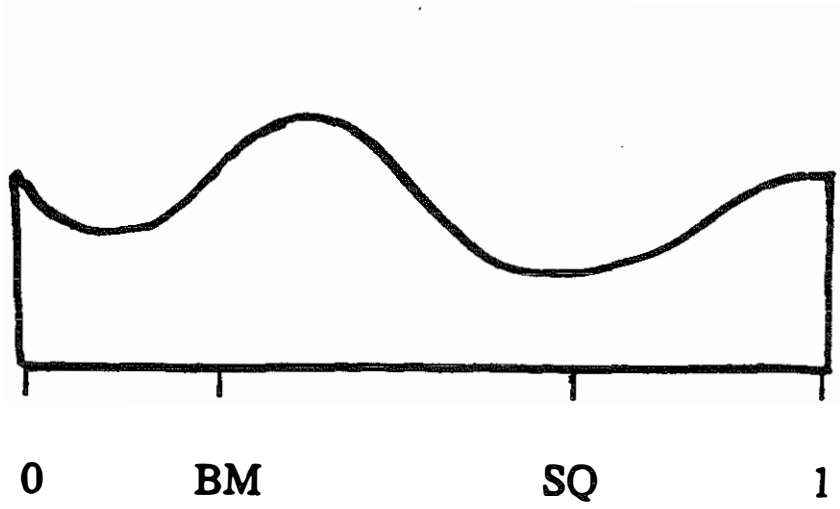
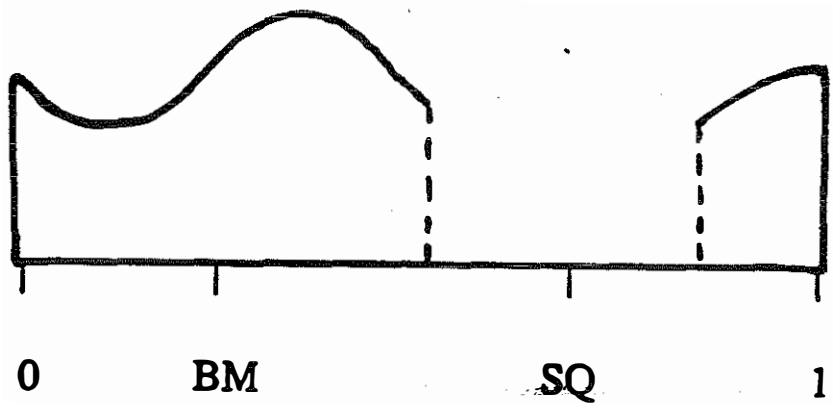


FIGURE 2:
Updated Beliefs from the Observation of Costly Effort

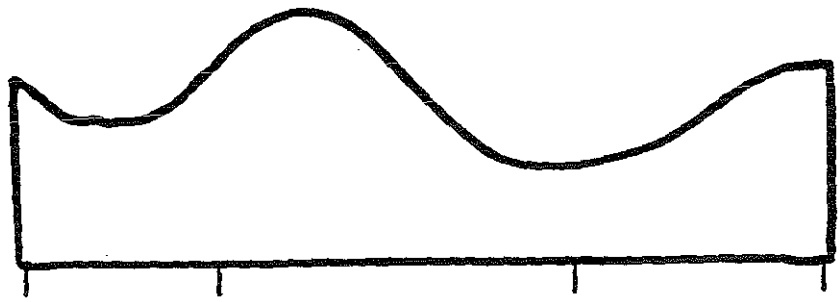


Voter Beliefs Before Observing Costly Effort

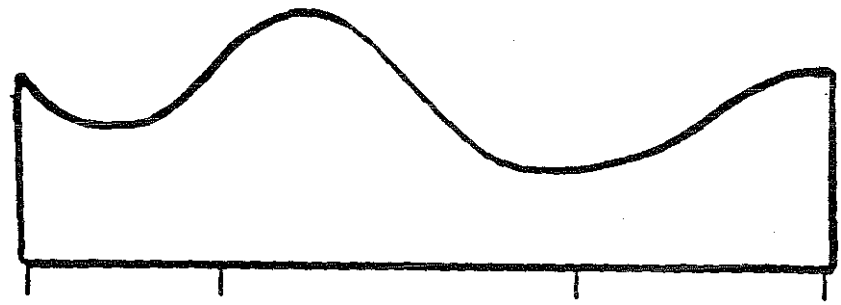


Voter Beliefs After Observing Costly Effort

FIGURE 3:
Updated Beliefs from the Observation of a Minimally Credible Campaign Message

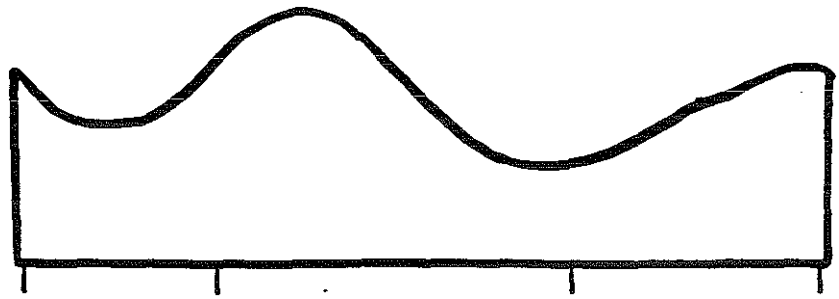


Voter Beliefs Before Observing M.C. Message

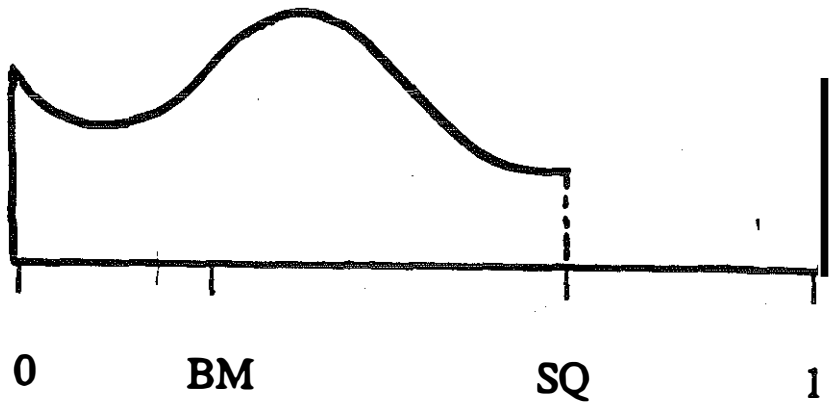


Voter Beliefs After Observing M.C. Message

FIGURE 4:
Updated Beliefs from the Observation of a Perfectly Credible Campaign Message

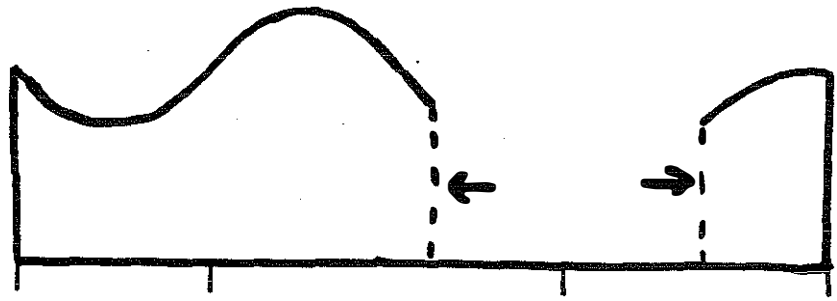


Voter Beliefs Before Observing P.C. Message



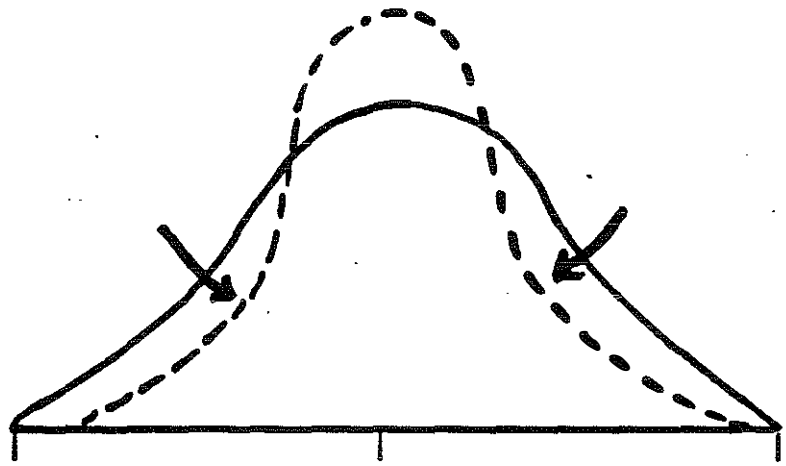
Voter Beliefs After Observing P.C. Message

FIGURE 5:
Relationship Between Theoretical and Empirical Concepts



0 BM SQ 1

Improved Voter Inferences



0 InformedVote 1

Higher Probability of Casting an Informed Vote

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